nature, full of information on the habits of a multitude of North American insects, good, bad, and indifferent, as to the characters borne by them. There are also several excellent woodcuts; yet we fancy some of these are old friends. In future numbers we hope to see more originality in this respect, because the constant reproduction of the same illustrations in different works, engenders a suspicion, with those uncharitably inclined, that the text may be sometimes written up to the illustrations, and the latter not made subservient to the former, as ought to be the case. We shall watch the progress of this journal with appreciative interest. The list of names of those who have promised occasional contributions includes most of the leading American entomologists.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

A Museum Conference

YOU did me the honour, about two years ago, of inserting an unsigned communication pointing out the extreme desirability of a conference of officials connected with museums and galleries of art throughout the country. At the time the subject received a good deal of attention from various quarters, and the numerous advantages which might be derived from such a meeting commended the suggestion to all who wrote on the subject. No one, however, ventured to make a practical move in the matter at the time, and the subject consequently dropped.

Further consideration and growing experience have deepened my conviction of the utility of the conference scheme; and as I have reason to believe I am not singular in that experience, I desire now to see some effort made to bring the question to a practical issue. With this view I shall be glad to co-operate with other museum officials who feel inclined to take part in the preliminary work of organising a conference of those interested in museums and art galleries. As to where, when, and how the conference should be held, I do not wish to offer a single suggestion which might anticipate future consideration. Neither do I consider it necessary to occupy your space with any statement as to the great and manifold advantages which ought to accrue to our scattered exhibitional institutions by a union such as might be formed. These are surely too manifest to every individual who has to do with any museum, especially in the provinces.

I hope this question will now be taken up heartily and energetically by all interested; and while I would beg that you may give space for the suggestions which others may wish to make through the medium of NATURE, I shall be glad to enter into correspondence with those who may address me privately.

Kelvingrove Museum, Glasgow JAS. PATON

The Himalayan Ranges

I HAD not intended to notice Mr. Trelawney Saunders's remarks on Mr. Medlicott and myself as the authors of the "Indian Geological Manual" (NATURE, vol. xxi. p. 96). As, however, Mr. Medlicott's reply (*ante*, p. 301) has been misinterpreted by Mr. Saunders, and as the latter has, in his rejoinder (*ante*, p. 347), brought a specific charge of omission which can, I think, be shown to be unfounded, against a portion of the "Manual" written by myself, I am obliged to take part in the discussion.

In Mr. T. Saunders's original paper (*l.c.*, p. 96) read before the British Association, two objections were raised to the views on physical geography adopted by the authors of the "Manual." The second of these objections referred to the limits of the Himalayan range, which we did not represent as extending west of the Indus. Mr. Saunders must have read very little of the "Manual," or he would have seen that this limit was not absolutely defined; on the contrary, at p. 518, it is expressly termed provisional. As Mr. Medlicott has shown, there is a good geological reason for the limit adopted; but another cause, of perhaps even more importance, is that this limit coincides with the boundaries of the area described in the work. I cannot enter into the question here, but the fact is, there are just as good reasons for making the Himalayan range terminate at the Jhelum, if not even farther east, as for prolonging it beyond the Indus.

The first objection was couched in much stronger language. Mr. Trelawney Saunders had represented the Himalayas as consisting of two chains; we were accused of having adopted an "antiquated theory." No reference was given, but from the context it was evident that this "antiquated theory" consisted in representing the range on a skeleton map by a single line along the water-shed or water-parting (I will employ the latter term to prevent any risk of misconception). Mr. T. Saunders says (*l.c.*, p. 96) that they (*i e.*, Mr. Medlicott and myself) "do not condescend to any reason for this conclusion." This is not quite correct. If Mr. Saunders had "condescended" to read the two and a half pages in the introduction of the "Manual" relating to the physical geography of the Himalayas, he would have found a reason on p. x.

Mr. Medlicott very justly pointed out that the reason for omitting the representation of a second chain was due to the irrelevancy of the question whether there are one or two chains to the matter in hand, that is, to the physical geography of India as related to the geology. Mr. Saunders has quite misinterpreted Mr. Medlicott's meaning when he says (p. 348): "Mr. Medlicott contends that the omission was due to the irrelevancy of the great range to the matter in hand." Of course Mr. Medlicott means nothing of the kind.

In his letter just referred to, Mr. Saunders writes thus:—

"But my complaint was based, not on my delineation, but on a trigonometrical survey, and it was caused by a *description*, not of the geology, but of the physical geography of India, in connection with a map of its hill-ranges, that has nothing geological about it. It is in this expressly geographical part of the 'Manual' that I find the greatest range of snowy peaks in the world *omitted from a geographical notice* and delineation of the Himalaya."

The italics are mine. Again no reference is given, but the remarks quoted can only apply to the description of the physical geography, accompanied by a skeleton map, in the Introduction to the "Manual." In this description the "geographical notice" of the Himalayas occupies barely two and a half pages. One would have thought that before writing the sentence I have quoted the writer would at least have read this small amount of letterpress. Yet I scarcely think Mr. Trelawney Saunders can have done so, or he could scarcely have overlooked the following passage at the bottom of p. ix. and upper part of p. x.

"Many geographers distinguish two parallel ranges from the neighbourhood of Simla to the eastward: the snowy range proper, formed of the highest peaks; and a more northern ridge, forming the water-shed between the Tibetan plain and the rivers running to the plains of India."

To save space I quote no more, but I am convinced that any one who will refer to the two and a half pages headed "Himalaya," in the Introduction to the "Manual," will see that Mr. Saunders is quite in error in saying that the main range is ignored.

As Mr. Trelawney Saunders has not understood Mr. Medlicott, I can only hope that the following explanation may be clearer:—

In his original paper and in that in the *Geographical Magazine* for 1877, pp. 175, 176, Mr. Saunders contends that the Himalaya south of the Sanpu and upper Indus consists of two "chains" (these are alternately called chains and ranges). The southern chain is formed by the line of great peaks, the northern by the water-parting between the drainage areas of the Upper Indus, Upper Sutlej, and Sanpu on the northern side, and various rivers running to the plains of India on the southern.

Now it is manifest that this division of the Himalayas into two chains is due to the fact that two different, and to some extent irreconcileable, definitions are adopted for the term "chain" in the two instances. Mr. Saunders's southern chain is a line of great peaks, but is not a continuous water-parting; his northern chain is a continuous, or almost continuous, water-parting, but is not a line of great peaks. It has never been shown that the two are distinct axes or lines of elevation; on the contrary, all the evidence we possess tends to show that both are due to one great fold of the earth's surface, and until these northern and southern chains are shown to be of diverse origin, it is perfectly reasonable to decline to accept the two distinc acceptations of the term "chain," and it is consequently perfectly correct

to represent both on a skeleton map as constituting one great range or axis of elevation. The sub-Himalayas consist of rocks of different age from those of the Himalayas, and there is some reason for believing these hills to be of later origin than the main chain; they are therefore represented in our map as a distinct range.

It would take too much space to criticise at any length Mr. Trelawney Saunders's Tibeto-Himalayan system (*Geographical Magazine*, 1877, p. 173). This system proposes to resolve "the leading outlines of the vast mass of which it treats into four great chains, with their outer slopes and intermediate valleys or plateaus." The chains are called the Kuenluen, Karakorum-Gangri, and Northern and Southern Himalaya. Now the greater part of the Tibetan area, including, at all events, all east of the meridian of 82° E. long., is too imperfectly known for any positive assertion to be made as to the number of ranges. In the better known western part of the area one fact alone, the omission to include as one of the main structural features, the range between the Indus and Shayok, shows the description and delineation to be geographically incorrect. The range omitted is at least of equal importance with some of those included. There are many other points open to question, such as the representation of the ranges north and south of Cashmere, as mere continuations of the so-called Northern and Southern Himalaya. In short the system will not fit into the only part of the area with which we have any adequate acquaintance. The accompanying map is doubtless an admirable sketch of the Himalayas as they would be if reconstructed according to Mr. Trelawny Saunders's hypotheses, but I think all who have ever been in those mountains will agree with me that it is not an accurate representation of the range as at present existing.

In conclusion I must decline to reply to any further remarks on this subject from Mr. Trelawney Saunders. It appears to me that Mr. Medlicott and I are entitled to express an independent opinion on the physical geography of the Himalayas without being accused of adopting an antiquated theory. In addition to the geographical data known to Mr. Saunders we have some acquaintance, imperfect, it is true, but still of importance, with the geology, and we have both some slight personal knowledge of portions of the range. Under these circumstances we have not adopted the theory advocated by Mr. Saunders simply because we consider it not supported by sufficient evidence.

February 29 W. T. BLANFORD

[This correspondence must end here.—ED.]

Tidal Phenomenon in Lake Constance

LES mouvements de la glace et de l'eau du lac de Constance décrits par M. S. J. Capper (NATURE, vol. xxi. p. 397) ne doivent pas être rapportés à une marée luni-solaire, ce phénomène étant inappréciable sur un lac dans si petites dimensions. Je me fonde sur les résultats négatifs que j'ai obtenus sur le lac Léman, plus grand en longueur et en surface que le lac de Constance. En utilisant les tracés de mon limnographe de Morges qui me permet d'évaluer à chaque instant à un millimètre près, la hauteur du lac en choisissant les circonstances les plus favorables, calme absolu de l'eau, et époques de syzygie, je n'ai jamais pu reconnaître de traces de marées luni-solaires.

En revanche les mouvements de balancement de l'eau que nous étudions depuis bien des années sous le nom de *seiches*, expliqueraient facilement une partie des faits signalés. Les seiches, comme on le sait, sont un mouvement de balancement de toute la masse du lac, qui oscille d'une extrémité à l'autre comme le fait l'eau d'une cuvette ou d'une baignoire. Il est vrai que le rhythme des seiches du lac de Constance, pour autant que je le connais par une seule observation du 14 septembre 1874, n'a qu'une durée d'une heure environ, et non douze heures comme l'indique le batelier de M. Capper. Il serait fort à désirer, pour l'interprétation de phénomène, que M. Capper put fournir des données et observations aussi exactes que possible des mouvements qu'il décrit. F. A. FOREL

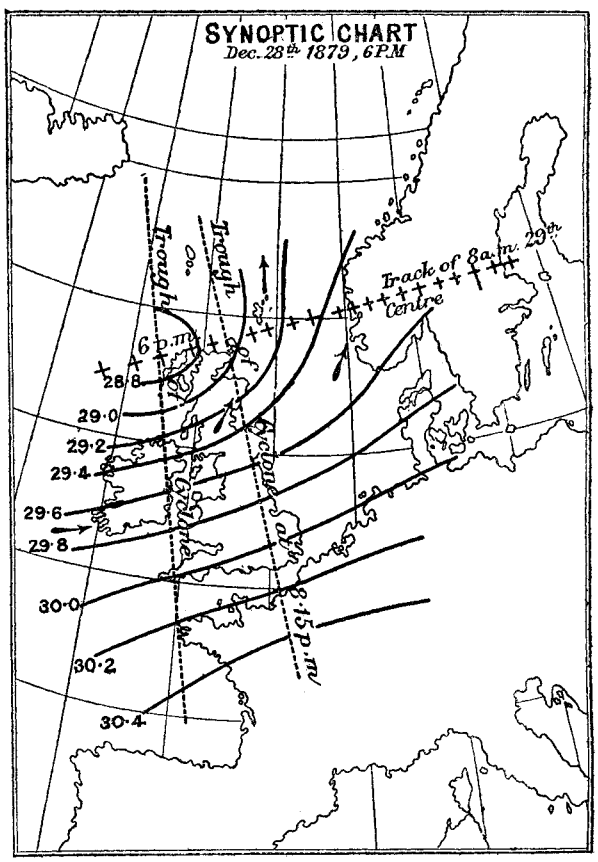
Morges (Suisse), 3 mars

The Tay Bridge Storm

A BRIEF account of the results obtained from the examination of a large number of observations referring to the storm on December 28, 1879, may be of interest even to your non-meteorological readers.

At 6 P.M. on the evening of that day, as will be seen by the accompanying chart, Fig. 1, the centre of a cyclone of considerable intensity was situated close to Stornoway. By 8 A.M., the 29th the centre had moved a distance of about 800 miles to the vicinity of Stockholm, which gives the high mean velocity of 58 miles an hour. But by a method detailed below, it is found that between 6 P.M. and 8·15 P.M. the centre moved along the north of Scotland at the rate of 62 miles an hour, which is, I believe, the highest on record in this country. No precise relation has yet been traced between the velocity of a cyclone centre and the strength of the wind in it. In any part of a cyclone the velocity of the wind is undoubtedly principally dependent on the closeness of the isobaric lines, but there is a good deal of evidence to show that when the velocity of the centre is very great, the strength of the wind for any given gradients is increased, or at all events becomes more squally and gusty.

In this case the steepest gradient was down the west of Scotland, but only amounted to about 0·13 inch per 50 miles, which is a very moderate amount for a winter storm.



An important result of recent research has been the discovery that every cyclone is divided into two parts by a line drawn through the centre, more or less at right angles to the direction of its motion, at all points in front of which the barometer is falling while it is rising in rear. This line marks out what is called the trough of a cyclone, and while the front and rear present marked contrasts both as regards the in-curvature of the wind, and still more as regards their physical appearance, it is also found that the passage of the actual trough all along its southern portion, except very near the centre, is marked by violent squalls. In the accompanying diagram the position of the trough at 6 P.M. can only be drawn approximately from general considerations as passing down the west of Scotland, but at 8.15 P.M. I have fortunately been able to locate it with great accuracy. At that time the barometer turned upwards at Wick, and almost at the same moment my own barograph in London also turned upwards with the characteristic squall. The line of the trough joining those two points would then be about thirty-three miles east of Dundee, and by combining it with the previous data, the high centre velocity of sixty-two miles an hour was obtained.